

History of the Speaking Telephone

As the writer of the article on the history of the telephone, to which so eminent an authority as Prof. Watson takes exception in the long and interesting letter he has contributed to your columns, perhaps you will allow me to say a few words. Prof. Watson expresses his "astonishment at the claim now made that he (Mr. Gray) anticipated Mr. Bell in the invention of the speaking telephone," and speaks of the "erroneous statement of facts" contained in the article in question (*NATURE*, vol. xviii. p. 696). Unfortunately Prof. Watson has not specified the statements which are erroneous, and appears to have overlooked the fact that the article is a review of the works of Mr. Prescott and M. du Moncel on the telephone, and that the "statements of facts" are chiefly quotations from those works. At the same time, using all the materials within my reach, careful inquiry had led me to concur, and in that article I expressed my concurrence in the following opinion, quoted from Count du Moncel's book:—"Si M. Bell a été le premier à construire et à rendre pratique le téléphone parlant, M. Elisha Gray avait le premier conçu le principe de cet instrument."

Gray and Bell were both exhibitors at the Philadelphia Exhibition, and Prof. Watson, writing as one of the judges of the scientific instruments exhibited, shows that whilst Gray merely submitted to the judges an apparatus for the multiple transmission of musical notes, and no speaking telephone, Bell not only exhibited a speaking telephone, but towards the end of June (1876) the judges, Prof. Watson and Sir William Thomson, obtained with Bell's instrument the clearest evidence of the electric transmission of speech; ¹ whereupon Mr. Gray was both surprised and incredulous, and even after the publication of Prof. Bell's discovery, he delivered a lecture exhibiting his musical telephone, but making no mention of a speaking telephone.

If the Philadelphia Exhibition were the only means for scientific publication during the year it existed, Prof. Watson's letter would effectually dispose of Gray's claim. An exhibition, however, is not the place for conceptions, but for accomplished facts, and I believe no one denies that to Mr. Bell is due all the credit of having been the first to construct, and that entirely independently of Gray, an articulating electric telephone. Gray's claim, as I take it, rests on his having registered in the American Patent Office, on February 14, 1876, "a means of transmitting and receiving vocal sounds telegraphically," and the drawing he gives of his invention shows a correct appreciation of the true principle of an articulating telephone, to which his previous researches had been gradually leading him.

I should be sorry to appear in any way to depreciate the splendid achievement of Prof. Bell through having referred to other workers in the field of electric-telephony. In fact up to the time the article in *NATURE* appeared, I fear that, through ignorance, I had done but scant justice to Mr. Gray, having attributed the conception of the principle of an articulating telephone solely to Prof. Graham Bell.

There are two points in the history of the telephone upon which I should be very glad to have authoritative information from Prof. Watson or other of your American readers; the first relates to the claim made by Prof. Dolbear, and the second to the introduction of the ferrotypic diaphragm. W. F. BARRETT

Royal College of Science, Dublin, December 9

The Formation of Mountains

In the account of M. Favre's experiments in *NATURE*, vol. xix. p. 103, I find the following passage:—"It is, in fact, very probable that our globe is at the stage when, according to Élie de Beaumont, 'the mean annual cooling of the mass exceeds that of the surface, and exceeds it more and more.' It must follow that the external strata of the globe, tending always to rest on the internal parts, are wrinkled, folded, dislocated, depressed at certain points, and elevated at others."

The whole theory of these dislocations, &c., thus depends on the assumption that the interior of the globe is cooling more rapidly than the crust. This has always seemed to me an impossibility, and even an absurdity, and I shall be very glad if any of your correspondents will explain how it is possible. I have always understood that the surface of the earth does not

now derive any appreciable portion of its heat from the interior; but if the interior is cooling rapidly, to what can it part with its heat but to the crust? Volcanoes and hot springs no doubt allow a certain portion of heat to escape, but it must be an infinitesimal part of the heat of the entire mass. If the meaning of the statement is, that the heat received from the sun now keeps the surface at a permanent mean temperature, quite irrespective of central heat or cold, and that therefore the loss of heat by volcanoes, &c., causes the centre to cool while the crust does not—this may be admitted, but it is doubtful whether it can have any bearing on the effects observed. For, on this theory, all the compression would take place in that shallow superficial layer which is kept above its normal temperature by the sun's radiation; and as we go back into past time this superficial layer would be thinner and thinner. But all geological evidence goes to show that folded and contorted rocks were subject to compression at considerable depths; and further, that such contortion was greater in comparatively early than in very late geological times—both facts directly opposed to the theory in question. Will any one of our great physicists enlighten us?

ALFRED R. WALLACE

AFTER reading your *résumé* of Prof. Alphonse Favre's interesting experiments on the formation of mountains by lateral thrust, it occurred to me that it would be easy to devise a mode of experimenting which would more nearly correspond with what takes place in nature. In M. Favre's experiments the lateral thrust was simply in one direction. In nature it is in all directions.

If a disk of india-rubber were stretched by means of a steel ribbon bent into a circular spring, on letting the spring slowly recoil there would be a lateral contraction of the india-rubber in all directions. A layer of clay upon that disk would, I think, show not the transverse inequalities of M. Favre's drawings, but a diversified unevenness more nearly resembling the actual surface of the earth.

ARTHUR RANSOM

Leicester, December

New Galvanometer for Strong Currents

I OBSERVE in *NATURE* (vol. xviii. p. 707) an article on a new galvanometer for strong currents by Mr. Eugen Obach. I published a paper on the same form of galvanometer seven years ago, and inclose a copy of my paper which was published in the *American Journal of Arts and Sciences*, vol. ii., August, 1871.

JOHN TROWBRIDGE

Harvard College, Cambridge, Mass., U.S.A., November 23

Explanatory

I MUST ask you, in common fairness, to allow me to protest against P. G. T.'s mistaken statement (vol. xix. p. 71) respecting a sentence which he quotes without the explanatory context. The moving force exerted by the earth on the moon as a whole is of course precisely equal to the moving force exerted by the moon on the earth. I had not to learn this from P. G. T., but had said so in so many words. But the moving force exerted by the earth on a given amount of matter in the moon is eighty-one times greater than the moving force exerted by the moon on an equal amount of matter in the earth. P. G. T. will scarcely deny this, and he cannot deny that the whole statement from which he quotes one sentence meant this, and this only. Nor, if he did, would any one who has read the chapter on the moon's motions in my treatise on the moon, believe such a statement.

He quotes a passage from my last book without comment, but, unfortunately, not without serious alteration. Apart from the undue emphasis which he thus gives to certain parts of it, the passage expresses my honest opinion. That I may be mistaken is quite possible. Men are always misunderstanding each other. If I find I have erred, I will acknowledge as much.

Until the word "heat" ceases to be used in common speech in two senses, or I am shown that when used for "temperature" (as when we say blood heat, boiling heat, a heat of 90° F., and so forth), it can be understood to mean "caloric," I intend always to use it in familiar writing about science. I deliberately struck out the word "temperature" wherever I had used it, and replaced it by the word "heat," in the same way and for the same reason that I often replace the word "velocity" by the

¹ I am glad to learn the exact date of the trial in question, which was given as August in the article.

word "speed." If in any passage ambiguity has thus been occasioned—or, as I would rather say, if anything I have thus said can be mistaken—I shall be glad to hear of it and set it right.

I must have failed, however, to make my meaning clear to P. G. T. in pp. 194 and 240. If at least he rightly understands me, I must leave him to settle with observed facts in one case and with the recognised authorities in the other.

My account of the earlier experiments of Professors Andrews and Tait was taken, as stated, from a paper by Prof. Heaton. P. G. T. ought to know the facts, and I accept his correction. When my article was written, several years ago, the "now received idea" was not yet received. I did not err in calling that theory "beautiful" and "ingeniously conceived" which is now generally accepted. But if I had, it is a less serious mistake to describe a sound theory as still open to doubt, than to describe a doubtful theory as demonstrated. This the author of the sea-bird theory of comets might remember with advantage.

RICHARD A. PROCTOR

Graphic Granite

I HAVE been spending some time of late in the examination of the rocks of this district, and was pleasantly surprised, a few weeks back, at finding some well-marked specimens of graphic granite among the waste material raised from Huel Agar Mine. It very closely resembles that found at Portsoy, N.B., but the felspar is grey instead of red. As I am not aware that this interesting rock is known to exist in any other locality in England, the observation may be worthy of record.

W. End, Redruth, December 2

FRANK JOHNSON

The Phonograph and Vowel Sounds

IN the interesting paper on "The Phonograph and Vowel Sounds" (vol. xviii. p. 340, *et seq.*), the authors remark that although the general results are the same as I have inferred from my own researches, the special numbers expressing the distribution of total intensity of vowel sounds among the partial tones are very different. Perhaps you will have the kindness to communicate to your readers the following reasons explaining, as I believe, the differences mentioned above.

1. The tables given by the authors, which contain the distribution not of intensity but of amplitudes, must be altered in a manner readily seen in order to be comparable with my tables.

2. The marks impressed by the phonograph contain certain peculiarities which, although without influence on the tones spoken from the instrument, remain effective in modifying the form of the curves obtained by mechanically transferring them.

3. The objective intensity (kinetic energy) determined by the authors is nearly, but not quite, proportional to the subjective intensity (quantity of sensation) which I have measured with the aid of resonators.

4. As I have observed, the differences of English and German pronunciation cause remarkable differences in the distribution of total intensity of vowel sounds among the partial tones.

Taking the above points into consideration it will be seen that the differences mentioned by Messrs. Jenkin and Ewing appear much smaller.

Besides I am pleased to notice that the authors, like myself, consider the flexibility of mouth cavity as important in explaining, where it exists, the characteristic pitch and other properties of vowel sounds.

F. AUERBACH

Local Colour-Variation in Lizards

THIS subject has recently been very fully discussed by my friend, Dr. Max Braun, assistant in the zoological laboratory of the University of Würzburg. His paper, which has especial reference to the lizards of Minorca and of some of the smaller islets of the Balearic group which lie round that island, is entitled "*Lacerta Lilfordi* und *Lacerta muralis*," and will be found in Part I. of the fourth volume of Prof. Semper's "Arbeiten aus dem zoologisch-zootomischen Institut in Würzburg," published in May, 1877.

Braun refers constantly in this paper to a memoir by J. von Beidraga, entitled "Die Faraglione-Eidechse und die Entstehung der Farben bei Eidechsen," which was published at Heidelberg in 1876.

P. HERBERT CARPENTER

Eton College, December 9

The Range of the Mammoth

ON November 6 Prof. Boyd Dawkins read a paper before the Geological Society on "The Range of the Mammoth in Space and Time." As the professor and several other recent writers have taken it to be proved that *Elephas primigenius* occurs in pre-glacial beds, it will, perhaps, be as well at once to review the evidence.

Geologists often speak of "pre-glacial beds" when they only mean beds beneath some one boulder clay, perhaps No. 6, or even later in the list given below. The succession is roughly as follows:—

6. Hessel Boulder Clay	} Upper, Middle, and Lower of the North of England (?)
Hessel Gravel	
5. Purple Boulder Clay	
Bridlington Cray	} Upper, Middle, and Lower of Lincolnshire, &c. (?)
4. Chalky Boulder Clay	
Mid-glacial ¹	} Upper, Middle, and Lower of East Anglia.
3. Contorted Drift ¹	
Sands ¹	} Lower Boulder Clay of the Norfolk Coast.
2. Second Till ¹	
Intermediate Beds ¹	
1. First Till ¹	} Pleistocene.
Arctic Freshwater Beds ¹	
Temperate Freshwater Beds ¹	
(Land surface.)	
Weybourn Beds, estuarine, including the "Forest Bed." ¹	

As the lower boulder clay of Northwich, in Cheshire, appears to be No. 5 or No. 6, and consequently newer than the upper boulder clay of East Anglia, the molar of *E. primigenius* found beneath it need not be pre-glacial. The Hertfordshire boulder clay, beneath which Prof. Prestwich found a tooth is, I believe, No. 4.

In East Anglia I have seen two molars of *E. primigenius* from the contorted drift, No 3 in the list, but it has not yet been found lower. All the specimens said to come from the forest bed have been dredged or picked up on the beach, and are of no value as evidence. At Bacton, on the Norfolk coast, I dug out a jaw and three teeth of the mammoth from a post-glacial deposit; if the denudation of the cliffs had preceded these teeth would have been found on the beach mixed with those of *E. meridionalis*. There appears to be one specimen, and one only, found *in situ* in the Forest Bed which can with any probability be referred to *E. primigenius*; this was found some years ago by Mr. Savin, of Cromer, it has not yet been satisfactorily determined, but from its peculiarity and the difference of opinion about it, it appears certainly not to be the ordinary form.

CLEMENT REID

Egton Bridge, Yarm, Yorks

The Bunsen Flame a Sensitive Flame

IT is not generally known, if it has ever been noticed before, that the Bunsen lamp gives a flame sensitive to sounds. A lamp should be chosen which has a tendency to "burn below;" this may usually be secured by opening the air passages to the utmost and lessening the supply of gas. The flame should burn quietly. My most sensitive flame is four inches high; the gas at about one inch pressure of water. A smart tap with a penholder on a glass cylinder a yard from the flame causes the characteristic "ducking," which is sometimes so energetic as to extinguish the flame or to cause it to burn below. The acute sound of rattling bottles, of a glass rod against a beaker, and many such familiar sounds of the laboratory, are the most effective. This may explain burning below without obvious cause. A tap on a mortar with the pestle twenty feet distant from a well-adjusted flame causes it, and so, often unintentionally, we may have the same result.

W. W. HALDARE GEE

Preston, December 3

OUR ASTRONOMICAL COLUMN

JEAN DOMINIQUE CASSINI.—In the course of his examination of the older archives of the Paris Observatory, which had been placed at his disposal with unrestricted permission to make extracts for use in his lunar re-

¹ These will be described in the Geological Survey Memoir on the Cromer Cliffs